

think, still less creditable to the writer. Whatever be one's own views on the subject, the question of the tenability of the theory is still *sub judice*, and it is not becoming in "G.H." to speak so contemptuously of the author of the address for not taking the same view as he does of the merits of the controversy.

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On the Mode of the Transverse Propagation of Light

IN NATURE, vol. xxi. p. 301, is a letter by Mr. W. M. Hicks containing some critical remarks on a paper of mine, "On a Mode of explaining the Transverse Vibrations of Light" (NATURE, vol. xxi. p. 256), which I shall be glad to notice here.

Firstly, it is, no doubt, understood that the theory proposed by me cannot be regarded as *in opposition* to any existing theory, from the simple fact that no theory or clear conception of the constitution of the ether (in regard to the mode of propagation of the transverse vibrations of light) appears really to exist. The notion of the ether resembling a "solid" or an "infinitely thin jelly," cannot, of course, be regarded otherwise than as a resource in the face of a difficulty, which, however, we think must appear to any impartial inquirer to increase rather than diminish the difficulty; and therefore the inference would seem a not unreasonable one that any *true* theory of the constitution of the ether would be something totally different from "statical" theories of this kind. As it has been one of my objects to prove, after considerable attention given to the subject, that but *one* view of the constitution of the ether is in principle conceivable (or that one solution to the problem already exhausts the limits of the conceivable), I may therefore be excused for having some confidence in the fundamental groundwork (at least) of the view adopted, and am therefore all the more ready to reply to any criticisms on the subject, though no doubt (as in the case of any theory possessing points of novelty) difficulties may be expected at first to arise that may entail considerable thought to remove them. It need not be premised that the attainment of truth is the ultimate object of all.

In the first place, in regard to the remarkable means of correcting and adjusting their own motions that atoms moving freely among each other have been proved to possess, I may at once withdraw the expression "instantly," in regard to the rate at which this self-adjustment takes place. The expression is at best a vague one, and the idea arose from the known fact of the practically instantaneous adjustment that takes place in the case of an ordinary gas. The mean velocity of the ether atoms would, of course, be necessarily equal to that of light, and all that is essential is that the adjustment should be rapid enough to maintain adequately the equilibrium of the ether.

In regard to the second difficulty mentioned, I do not see that the fact of some of the atoms of ether moving at a greater or less velocity than the *mean* velocity (which is equal to that of light) should put a difficulty in the way of accounting for the regularity of the waves of light. For it has been proved in connection with the kinetic theory that the number of atoms whose velocities differ by any great proportion from the mean velocity is relatively very small. These atoms would no doubt distribute the energy irregularly over the beam of light, but the total effect would in this way neutralise itself. The great majority of the atoms would still be moving at the mean velocity and distributing the energy in regular waves, and producing that sequence of energy that we call light. I may note that in a paper on "The Mode of the Propagation of Sound on the Basis of the Kinetic Theory of Gases," published by me in the *Phil. Mag.* for June, 1877, and where a mathematical determination of the velocity of the wave was appended by the late Prof. Clerk Maxwell—the same considerations regarding the varying velocities of the atoms would be involved as above; and yet we know that as a fact the sequence of the waves of sound is in perfect regularity.

In reference to the third difficulty mentioned by Mr. W. M. Hicks, regarding the explanation of refraction and reflection. This leads me more strongly to return to a detail in regard to the constitution of the ether I had before adopted, but had not fully grounded, probably from the absence of the requisite encouragement to devote an adequate amount of thought to the subject. I quote the following in substance from a paper already written. I am led to regard the ether atoms as of *two* grades of dimensions. Of course there is no *a priori* reason why they should be all of one size, and the fact of their being of two sizes does not alter the principle of the theory in the least. They

may therefore be assumed, if facts require it, to be of two grades of dimensions. The one set of atom (specially concerned in the effects of gravity) are to be considered as enormously smaller than the atoms propagating light, and consequently their velocity (which will adjust itself automatically in the inverse ratio of the square root of their mass) very much greater. It might perfectly well be assumed (for example), that the mass of the atoms producing gravity is such that their velocity equals, say 10,000 times the velocity of light. I would just remark, in connection with this, that the expression "wonderful" sometimes applied to the velocity of light is, I think, to be deprecated. I would submit that there is nothing really "wonderful" in any velocity, because, however great a velocity is, it is always indefinitely small compared with that which it might be conceived to be, as one has in strict logic no power to limit arbitrarily the conceptions in this respect. If, therefore, there be reason for inferring a certain velocity to exist (no consequence what its value), it seems to me there is no ground for assuming it to be "wonderful." If a body or atom moves in free space without obstruction, there is nothing to curb its velocity, and its energy may even become immeasurably small at this velocity, provided the atom itself be small; and, in the same way, we have nothing to limit our conceptions as to the smallness of atoms. There can be no difficulty whatever in these conceptions, as mechanical principles are admittedly independent of *scale*, and therefore there is nothing mysterious whatever in the subject. The real mystery surely attaches to the spiritualistic assumptions about "forces" which spoil the interest of physical inquiries, and have involved that magnificent physical agent, the ether, in such a labyrinth of spurious mystery as to repel the inquirer. I cannot avoid the inference that any one who reflects seriously and impartially on the subject, will be disposed to admit that there really cannot be *two* methods in physical science, but only *one* method (the dynamical), the so-called "statical" speculations about "forces" leading nowhere. It has been proved again and again in connection with science that the so-called spiritualistic "method" is utterly barren, and only involves one in an inextricable maze of speculation from which there is no escape. I have thought these few remarks necessary in view of the special subject with which I am dealing.

It will be observed that the whole of the dynamical effects above referred to are automatic. The correction of the motion of the atoms so as to move in the right way to produce gravity and light is automatic; the adjustment of the relative velocities of the atoms between the two sets is automatic, or we make no arbitrary postulate at all. The effect of an adequate velocity for the smaller set of atoms would necessarily (from well known dynamical principles) cause them to oppose no measurable resistance to the molecules of gross matter vibrating in them, and consequently they could take no measurable part in the propagation of the energy of light. They would, on the other hand, produce an enormous pressure (adequate for gravity) on the molecules of gross matter—the pressure being as the *square* of the velocity.¹

The main reasons for assuming that the atoms producing gravity and those producing light are separate, are first the great pressure requisite for gravity, and the consequent necessity for an adequately high velocity to produce this pressure, and secondly (as Mr. W. M. Hicks points out), there would appear to be a difficulty in explaining the reflection of light from some bodies, and also the phenomena of refraction, if we assumed the atoms propagating light to pass through all bodies with perfect facility, as is necessary in the case of the atoms which produce gravity. As this letter has already grown to some length, I will at present confine myself to this inference, reserving some ideas relative to polarisation (in connection with the present theory) to a subsequent letter.

To prevent misconception, the fact may be cited that the above kinetic theory of the ether does not represent an *emission* theory of light. The motion of translation (which the ether atoms would possess if there were no light) merely serves as the carrier of the energy impressed upon them by the vibrations of the molecules of gross matter. On the other hand, the fact of the theory resembling (in the translation of the atoms) *one* of the ideas of Newton may possibly be regarded as rather a recommendation than otherwise. If, however (as I have certainly set

¹ If observation shows light to suffer no (sensible) diminution of velocity at reflection, it would follow that the luminiferous atoms do not suffer a (sensible) diminution of their transitory motion; rebound from gross matter, and consequently these particular atoms could not be appreciably concerned in the effects of gravity.

myself the task to prove) there is not really more than *one* explanation of the constitution of the ether in principle conceivable [excluding, of course, the essentially endless vagaries about "forces"], then on this ground alone the hope may be entertained by those who look to the existence of an *explanation* for every physical fact, that difficulties that may naturally present themselves at first will not prove insurmountable by a due amount of thought and careful analysis.

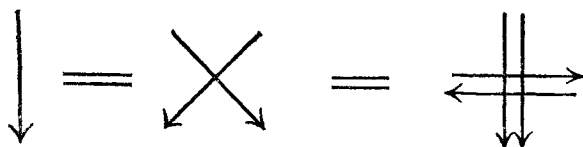
S. TOLVER PRESTON

The Transverse Vibrations of Light

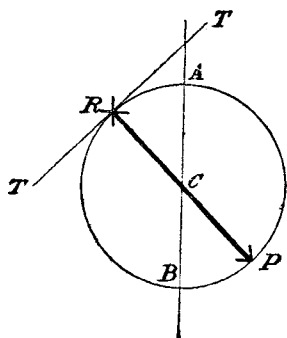
FEW are probably likely to underrate the vast benefits which have accrued to physical science since the time of Poisson, from the application of mathematical analysis to physical problems; but it seems to me we are at present rather in danger of forgetting that such mathematical reasoning can only lead to useful results when founded upon definite physical conceptions. It was upon such a basis that the triumphs of Young, Fresnel, and Airy were won; and it is for want of such a basis that I fear we shall get little aid from Mr. Tolver Preston's ingenious speculation. Mr. Hicks has taken some exceptions to them, which seem sound if he has correctly read the theory, though I am not quite sure he has, or that I should go quite with him in regard to what Mr. Preston *may* mean. But I wish to point out, with your permission, objections of a more simple and definite physical nature.

The only transverse movements capable of being communicated to an ether-molecule by transverse vibrations of matter, which do not involve translation through a measurable though minute distance in space, appear to be rotation on an axis, or (if we conceive the molecule as an annulus) alternate contraction and expansion—"vibration" Mr. Preston calls it—within its own limits. In the case of a gaseous constitution, transverse translation in space with the assumed long free path, must continue, and result in a free path different from the assumed direction of the ray. The particles of sand employed to illustrate the subject thus acquire a continuous transverse *motion* in passing through the sieve, and do not "vibrate" or come back in an orbit. Such true vibratory motion is the main characteristic of solid bodies, and is the best known reason for conceiving the ether as of a "solid" constitution.

What I wish to point out is, that large classes of phenomena appear to demand such actual transverse *motion* in orbits of the entire ether particles, and cannot at present be explained without. I confine myself to two of the simplest examples from polarisation. It is well known that the production of complementary colours from a plane polarised ray by a doubly-refracting film and analyse, may be simply represented to the eye and the mind by the following diagram equation, which shows the resolution of actual *motions*.



Similarly, the two circularly-polarised rays in quartz, and their conversion on emergence into a plane ray rotated on its axis by the angular value of the difference in velocity, may be represented thus:—



Here actual motions in the plane AB are resolved into two opposite circular motions represented by the doubly-barbed circle, which meet on their emergence at the point R, to which their

respective velocities from the common departure A have brought them. There they are again resolved, the two tangential forces TR destroying each other, and the two radial forces, RC, uniting in the rotated plane wave, RP. Here again we have throughout actual motions, through definite distances. And I am at a loss to see how whole classes of phenomena of which these are typical can be explained in any other way, or by any but a true "vibration" bodily to and fro in space. If it be so, then it is not enough for some vague physico-mathematical notion to satisfy abstract mathematical conditions; we must ask for the definite physical conception which is to account for the physical phenomena. Until we have this we have made no real advance in comprehending the physics of the ether.

At the same time I cordially agree with Mr. Preston in his regret at the comparative distaste for the study of this subject; and I may, perhaps, add a suggestion on my own part, though not really new. To my own mind it seems as easy to conceive of "matter" without gravity as with it, and of infinite elasticity as of elasticity at all (which is not easy). In ponderable matter, again, the most highly elastic solid bodies are as "solid" as the least; the greater mobility of their atoms by no means interferes with that peculiarity of vibrating *in orbits* and preserving a *locus* which distinguishes solids from fluids, and which so far we have been obliged by the phenomena to attribute to the ether also. Granted that to account for elasticity we have to conceive atoms not in contact, and are confronted by the old mystery of how they can act upon each other across a vacuum. Still, does this confront us any *more* in the ether than in ponderable matter; and, so far as they do go, are not our conceptions of the one sufficient for and equally applicable to the other?

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Diffusion of Copper in the Animal Kingdom

JE lis dans la NATURE, vol. xxi. p. 305, un article intitulé "Diffusion of Copper in the Animal Kingdom," se terminant par ces mots: *it is to be hoped that more extended observations will inform us of the exact nature of the rôle played by cupric compounds in the animal economy.* Je crois pouvoir satisfaire en partie au moins à ce vœu. Dans plusieurs communications insérées dans les publications des Académies des Sciences de Paris et de Bruxelles (1878 et 1879), j'ai montré que chez certains mollusques céphalopodes et gastéropodes et chez les crustacés décapodes, le cuivre joue dans le sang le même rôle physiologique que le fer dans notre sang.

Le sang veineux du poulpe (*Octopus vulgaris*), du homard (*Homarus*), etc., contient une substance albuminoïde incolore, cuprifère, à laquelle j'ai donné le nom d'*hémocyanine*, terme rappelant sa parenté avec l'hémoglobine. L'*hémocyanine* forme dans la branchie une combinaison peu stable avec l'oxygène; cette combinaison l'*oxy-hémocyanine* est d'un beau bleu. Elle se décompose en se dissociant sous l'influence du vide ou du contact avec les tissus vivants. Aussi le sang artériel du poulpe est d'un beau bleu tant que l'animal respire une eau bien aérée. Il suffit de comprimer la branchie, de gêner la respiration, pour voir le sang artériel se décolorer.

L'*hémocyanine* paraît avoir une constitution chimique analogue à celle de l'hémoglobine. Comme cette dernière elle est susceptible de se dédoubler en une substance albuminoïde ne contenant pas de métal et en une substance cuprifère qui paraît former des sels cristallisables analogues aux sels d'hématine.

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Liège, le 11 février, rue du parc, 25

Lines of Force due to a Small Magnet

I HAVE been recommended by Sir William Thomson to send you the following construction for the lines of force due to a very small magnet.

The equation to the lines of force due to a very small magnet placed at the origin of co-ordinates and lying along the axis of x is—

$$\frac{y^2}{(x^2 + y^2)^{\frac{3}{2}}} = \frac{1}{C} \dots \dots \dots (1)$$

By varying C we obtain a series of similar curves.

Transforming to polar co-ordinates by putting $x = r \cos \theta$, $y = r \sin \theta$, we get for the equation (1)

$$r = C \sin^2 \theta.$$